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E. J. STONE, Esq., Vice-President, in the Chair.

Boyd Moss, Esq., Clatford, Middlesex,

was balloted for and duly elected a Fellow of the Society.

On the Solar Parallax, derived from Observations of Mars at Ascension, in 1877. By David Gill, H.M. Astronomer at the Cape of Good Hope.

(*Abstract.*)*

Aided by a grant of 250*l.* from the Society, a like sum from the Government Grant Fund of the Royal Society, and with the loan of Lord Lindsay's Heliometer, Mr. Gill undertook an expedition to the Island of Ascension for the purpose of determining the parallax of the Sun, from observations of the diurnal parallax of *Mars*, during the favourable opposition of that planet in the year 1877.

After a brief history of the expedition, Mr. Gill describes the methods and instruments used, and gives the results of the operations by which the latitudes and longitudes of the observing stations were determined.

The stars used for comparison with *Mars* were observed in the meridian at thirteen of the principal Observatories. The result of a discussion of all these observations is given, and the proper motions are deduced from a discussion of all the old observations. A series of very accurate *preliminary* places is thus formed for comparison with the results of the heliometric

* The Memoir is being printed, and will appear in the next volume of the *Memoirs*.—ED.

triangulation. In the latter operation a night's work was so arranged that first a *standard distance* was measured, next three other distances in the triangulation, and then again the *standard distance*.

This *standard distance* was taken to be the angle between the stars $e-g$ for 1877·0. An approximate value of this angle (7054″·80) was adopted, the true value being assumed

$$7054''.80 + \Delta S.$$

From each measure of this *standard distance* the scale value was deduced in terms of ΔS, and from the different values so found the scale value was interpolated for the epoch of each observation. Thus in all the observations of the triangulation the deduced distances were affected systematically only by

$$\Delta S \times \frac{\text{measured distance}}{\text{standard distance}}$$

and by such errors as might exist in the heliometer by which the scale value would be different for the measurement of large and small angles. For example, if the radius of the cylindrical slides in which the segments move is different from the focal length of the object-glass, an error depending on the square of the distance would be produced.

Accordingly, each measured distance is supposed to be affected by a term d depending on the square of the distance, and by a term

$$= \Delta S \times \frac{\text{measured distance}}{\text{standard distance}}.$$

The tabular Right Ascensions of the preliminary star-places are supposed to be affected by errors that can be expressed by

$$ma + m^2b,$$

where m=the difference in magnitude of the star from 4·5 mag. (so that for 6th mag. $m=+1{\cdot}5$). The proper factors of these terms are formed for each comparison of the tabular distances (computed from the *preliminary places*) with the heliometer measures of the same distance.

In this way 70 equations are formed, involving the four unknown quantities

$$a, b, d, \text{ and } \Delta S.$$

Approximate assumptions are made as to the mean error of the preliminary Catalogue and that of the heliometer measure—the latter being derived from measures of 19 pairs of stars which have been observed more than once. The *mean error* of a heliometer measure was $\pm 0''{\cdot}22$ corresponding with a probable error of $\pm 0''{\cdot}15$.

A system of weights was adopted such that the mean error of an equation of weight unity should be $\pm 0''\cdot 50$, and the solution showed a mean error of $\pm 0''\cdot 55$, so that the weights originally adopted were considered sufficiently accurate.

The average weight of an equation was 2·3. The solution of these 70 equations by least squares, having regard to the weight of each equation, leads to four normal equations, and a discussion of these shows that the terms b and d have no reality, since their omission from the equations makes no sensible increase of the sum of the squares of the residuals; but the omission of the term a increases the sum of the squares of the residuals from 23·0 to 36·8. When allowance is made for the errors of the heliometer measures, it is shown that the square of the mean discordance between the heliometer and meridian observations is reduced as $2\frac{1}{2}$ to 1 by the introduction of the correction depending on magnitude. The value of the term so determined is

$$a = -0''\cdot 27 \pm 0''\cdot 07,$$

or, in other words, *the Right Ascensions of the preliminary Catalogue have to be diminished fully quarter of a second of arc per magnitude fainter than* 4·5.

Applying this correction to the preliminary Right Ascensions, the R.A.'s of each Observatory are separately compared with the corrected Catalogue so formed, each comparison giving an equation of the form

$$a + mb + m^2 d = \mathrm{C} - \mathrm{O}.$$

Normal equations are then formed, and from these the values of a, b, and d are found for each Observatory.

Applying the corrections thus deduced, the whole of the observations are rediscussed and a definitive Catalogue is formed, corresponding systematically with the heliometer observations.

[The remarkable result comes out, viz. that the eye and ear observations correspond, within the limits of probable error, with the results of the heliometer measures; but the chronographic observations require large corrections depending on magnitude.]

With the star-places so found, and with Le Verrier's Tables of *Mars*, corrected for an assumed constant error of $+2''\cdot 45$ in heliocentric longitudes (derived from the Greenwich Meridian Observations), and employing $8''\cdot 80$ for the Mean Hor. Solar Parallax and $\frac{1}{300}$ for compression of the Earth, the apparent tabular distances of *Mars* from the comparison stars are computed and compared with the heliometer measures. From this comparison equations are formed on the type

$$f'\Delta\alpha + f''\Delta\delta + f'''n + \nu z = \mathrm{O} - \mathrm{C} + rm - f'(\tau - \tau_0)\kappa + f''(\tau - \tau_0)\kappa';$$

where

f' and f'' are the factors in R.A. and Decl.

n, the unknown percentage of the error of the assumed parallax.

f''', the effect on the distance of increasing the parallax one per cent.

ν, $\frac{\text{The observed distance in seconds of arc}}{10000}$.

τ, the Greenwich mean time of observation expressed in days.

τ_0, the mean epoch of the combination to which each observation in that combination is reduced.

κ and κ', the daily rate of increase of the real over the tabular R.A. and Decl. at the epoch of observation.

These equations are then formed into combinations as follows:—

1. Each combination must consist of two or more *bunches* of observations. Those *bunches* must be symmetrical evening and morning, and must afford data for eliminating at least the scale value and parallax from their combination. *Bunches* are only to be considered corresponding and symmetrical when the comparison stars have been the same and have been observed in the same way in the evening and morning.

2. The mean epoch of each *bunch* must be within twenty-four hours of the mean epoch (τ_0) of the combination, since κ and κ' may legitimately be considered uniform during that period.

3. The observations within each *bunch* must be made without much interruption by cloud, &c., so that the scale value may be considered uniform throughout each *bunch*.

Subject to these conditions it was found that twenty-five combinations could be formed from the observations, but of these three, from unfavourable conditions, could not be included in the definitive result, though their introduction produces no sensible change in the resulting parallax or its probable error.

The following are the steps of reduction:—

1. Normal equations are formed for each *bunch*.

2. z is eliminated from the normals for each bunch, preserving the symmetry of the equations.

3. The normals of the same name in each combination are added together and so normals in $\Delta\alpha$, $\Delta\delta$, and n are found for each combination. From these $\Delta\alpha$ and $\Delta\delta$ are then eliminated, and an equation in n, with the coefficient of the true weight of n, is obtained for each combination, in terms of κ and κ'.

4. Neglecting κ and κ' an approximate value of n ($=-0{\cdot}25$) is obtained, and when this is substituted in the normals $\Delta\alpha$ and $\Delta\delta$, the values of $\Delta\alpha$ and $\Delta\delta$ are obtained for the epoch of each combination.

5. It is then found that the separate determinations of Declination may be represented by

$$\Delta\delta = -0''.66 + .0059\, t + 0.00077\, t^2,$$

(where t is the time, in days, from September $5^d.5$) with a probable error of only

$$\pm 0''.057$$

for each determination of declination.

But the Right Ascensions cannot be represented so accurately. There is a strangely marked tendency in the run of the values of $\Delta\alpha$ indicating a variation having a period of about 13·6 days. Some of the observations cannot be represented within reasonable limits without the supposition of such a curve, but the general run of the observations is expressed by

$$\Delta\alpha = +0''.44 - .0114\, t - .00112\, t^2 \qquad (1)$$

But *all* the observations are well represented by the expression

$$\left.\begin{aligned} \Delta\alpha = {} & +0''.43 && \pm 0''.07 \\ & + .0035\, t && \pm .0032 \\ & - .000367\, t^2 && \pm .00016 \\ & + \begin{pmatrix} 0.376 \\ \pm .071 \end{pmatrix} \times \sin\,(u + 122°.3 \pm 12°.6) \\ \text{where } u = {} & \frac{t}{13^d.626 \pm .067} \times 360 \end{aligned}\right\} (2)$$

and the probable error of each determination of $\Delta\alpha$ is $\pm 0''.199$.

[The coincidence between this period ($13^d.63$) and that of a term depending on twice the Moon's mean longitude is remarkable, but the author can find no grounds for such an inequality, nor does he consider the real existence of such a period to be as yet proved.]

6. The values of κ and κ' are then found for the epoch of each combination by the above expressions for $\Delta\alpha$ and $\Delta\delta$.

When κ is found by (1)

$$n = -.209, \quad \text{or } \pi = 8''.782 \pm 0''.0116.$$

When κ is found by (2)

$$n = -.251, \quad \text{or } \pi = 8''.778 \pm 0''.0119.$$

The probable errors of a single measure of distance are by (1)

and (2) respectively $\pm''\cdot234$ and $\pm''\cdot241$, or about double the probable error of measures of stars and minor planets.

The author then examines the various sources of systematic error.

It is shown, by terms carried throughout the discussion, that if *no corrections for refraction* whatever had been employed, the resulting parallax would have been increased by $+0''\cdot014$, and as Bessel's refraction tables cannot be supposed $\frac{1}{100}$ part in error, no conceivable error in the refractions can produce an error of $0''\cdot00014$ in the deduced parallax. This is due to the symmetrical arrrngement of the comparison stars, and it is shown by an independent discussion, that even when the error of scale value is not eliminated from every bunch, as in the definitive reduction, such is the general symmetry of arrangement of the comparison stars that the value of the parallax comes out the same, and with practically the same probable error.

Mr. Gill comes to the conclusion, after an examination of every source of systematic error, that the only systematic error to which the result can be supposed liable, may be a small error due to chromatic dispersion of the atmosphere, but he also shows that this is probably very small from the close agreement between the resulting parallaxes at great and lesser zenith distances.

The following are the separate results:—

When only symmetrical combinations are employed (definitive discussion):

From symmetrical observations combined ...	$8''\cdot777 \pm 0''\cdot013$
Rejecting 3 combinations observed under unsatisfactory circumstances... ...	$8\cdot782 \pm 0\cdot012$
Rejecting 3 combinations on the supposition of a periodic variation of the R.A.	$8\cdot778 \pm 0\cdot012$

When the scale value is supposed constant, its error not being eliminated as in the previous discussion:

From all symmetrical observations combined ...	$8''\cdot778 \pm 0''\cdot014$
Rejecting 3 doubtful combinations ...	$8\cdot784 \pm 0\cdot013$

When the whole of the observations are combined, and the star-places are regarded as absolutely known:

From all the observations combined	$8''\cdot783 \pm 0''\cdot026$
Observations of greater zenith distance.	$8\cdot786 \pm \cdot034$
„ lesser zenith distance...	$8\cdot780 \pm \cdot040$
„ before opposition	$8\cdot791 \pm \cdot034$
„ after opposition ...	$8\cdot772 \pm \cdot040$

The separate results of the definitive discussion are:—

Date.	π	Dif. from Mean.	Weight of n.
1877, July 31 E and M	8″·90	+0″·12	·113
Aug. 5 E and M	·65	− ·13	·086
12 E, Aug. 10 M	·80	+ ·02	·175
14 E, Aug. 13 and 14 M	·83	+ ·05	·180
15 E and M	·80	+ ·02	·079
16 E and M	·90	+ ·12	·091
19 E and M	·88	+ ·10	·109
24 E and M	·77	− ·01	·227
29 E and M	·84	+ ·06	·113
Sept. 4, 5, 6, E and M	·82	+ ·04	·547
6 E and M	·67	− ·11	·057
6 E and M	·62	− ·16	·097
7 E and M	·83	+ ·05	·060
8 E and M	·81	+ ·03	·134
8 E and M	·70	− ·08	·097
9 E and M	·83	+ ·05	·145
10 E, Sept. 9 E and M	·65	− ·13	·309
10 E 10 M	·66	− ·12	·106
19 E and M	·76	− ·02	·077
24 E and M	·71	− ·07	·109
27 E Sept. 25 M	·86	+ ·08	·147
Oct. 3 E and M	·81	+ ·03	·110

The definitive result of the whole discussion gives

$$\pi = 8''{\cdot}78 \pm 0''{\cdot}012,$$

which corresponds with 93,080,000 miles for the mean distance of the Earth from the Sun.

This value of π is compared with that from the recent accurate determinations of the velocity of light by Cornu and Michelson; when this velocity is combined, on the one hand, with the constant of aberration, and, on the other, with the time occupied by light in traversing a mean radius of the Earth's orbit as derived from observations of *Jupiter's* satellites.

Michelson's determination of the velocity of light combined with Struve's constant of aberration gives

$$\pi = 8''{\cdot}81.$$

Cornu's determination combined with the same constant gives

$$\pi = 8''{\cdot}80.$$

But the mean of all the best modern determinations of the constant of aberration gives, instead of Struve's value (20″·445), the somewhat larger value 20″·496, and Nyrén's discussion (*Mem. St. Petersburg Acad.* vol. xix.) of the best modern Pulkowa determinations (including Struve's) gives 20″·481.

Maclear's very accurate observations of the Declination of α and β *Centauri* at the Cape give 20″·53 and 20″·59 respectively for the constant of aberration, showing, so far as their evidence goes, that the constant of aberration is not less when determined from Southern than from Northern stars.

If, as other results would appear to show, Struve's constant of aberration should be increased by 0″·05, then the resulting solar parallax from the combination with this constant would be diminished by 0″·02, and the values of π would be

According to Cornu 8″·78,
Michelson 8·79,

values practically identical with the results derived from the heliometer observations of *Mars*.

Glasenapp's determination of the light equation (k) derived by him from eclipses of *Jupiter's* satellites gives

$$k = 500^{s}\cdot8 \pm 1^{s}\cdot02,$$

and this combined with Michelson's and Cornu's velocity of light gives

$$\pi = 8''\cdot76.$$

But if Glasenapp's determination of k is diminished in the direction of Delambre's value by the amount of its probable error—viz. 1^{s}—then the resulting parallax becomes

$$\pi = 8''\cdot78.$$

The large values of π which have been determined from meridian observations of *Mars* are explained by the fact that, at the altitudes at which the planet was observed in Europe in 1877, the limbs of *Mars* are spread by chromatic dispersion of the atmosphere into a fringe of coloured light 2″·2 broad. For the upper limb the violet part of the fringe is projected outside the disk of the planet, and for the lower limb the red part of the fringe is outside. In the field of a meridian instrument, illuminated by white light, more of the violet than of the red false limb would be obliterated, and hence the observer will cut more deeply with the spider-web into the false violet than into the more glaring red limb. Thus all the zenith distances measured would be too great, and the resulting parallax would also be too great. The results of other methods of determining the parallax

are considered, and the Memoir concludes with a statement of the methods which, in the author's opinion, should be adopted in future, for a definitive determination of the solar parallax.

Note on the Durations observed by the French Observers at the Transit of Venus, 1874. By E. J. Stone, Esq.

In the *Comptes Rendus* of the French Academy of Sciences No. 10, March 7, 1881, there is an interesting and valuable paper, by M. V. Puiseux, which contains the results of the contact observations made by the French observers at the Transit of *Venus*, 1874, December 8.

There were eight durations observed by the French observers at five stations: Pekin, Saint Paul, Nagasaki, Saïgon, and Kobé. The observers were Messrs. Fleuriais, Bellanger; Mouchez, Turquet; Janssen, Tisserand; Héraud; and Delacroix. The equations given by M. Puiseux lead at once to the following equations of condition for these eight observed durations:—

1	$x - 2{\cdot}94 \,.\, \delta\pi$	$= +0''{\cdot}41,$
2	$x - 2{\cdot}94 \,.\, \delta\pi$	$= +0{\cdot}03,$
3	$x + 2{\cdot}85 \,.\, \delta\pi$	$= +0{\cdot}61,$
4	$x + 2{\cdot}85 \,.\, \delta\pi$	$= +0{\cdot}35,$
5	$x - 2{\cdot}71 \,.\, \delta\pi$	$= -0{\cdot}87,$
6	$x - 2{\cdot}71 \,.\, \delta\pi$	$= -1{\cdot}11,$
7	$x - 1{\cdot}02 \,.\, \delta\pi$	$= -0{\cdot}21,$
8	$x - 2{\cdot}87 \,.\, \delta\pi$	$= +0{\cdot}82.$

In the formation of these equations the mean equatorial solar parallax has been assumed to be equal to $8''{\cdot}86$, and $\delta\pi$ is the correction which that value requires.

The agreement among these equations is not so close as all astronomers must have wished, and most astronomers may have expected, from the care bestowed upon the instrumental equipment of the different expeditions and the skill of the astronomers employed; but except the equations (5) and (6)—of which more hereafter—it may be seriously doubted whether the discordances are not such as must be accepted in such investigations. The error of each equation is the algebraical sum of the errors made at the two contacts.

If we determine the value of $\delta\pi$ from these equations, as they stand, on the assumptions that all the observations are equally good, and that the results are comparable *inter se*, or free from systematic errors, we shall find for the value of the solar parallax

$$8''{\cdot}96 \pm 0''{\cdot}19\epsilon.$$

Here ϵ is the probable error of the difference between a

distance of the centres of *Venus* and the *Sun*, at contact, and the mean distance between the centres, for all the contacts which are combined in the discussion.

The number of equations combined together is not sufficient for any great reliance to be placed upon the resulting probable error. But with a value of ϵ as large as $0''.5$, or with an assumed probable error of fifteen seconds of time in the observation of the contacts, the theoretical probable error of this result would not exceed $0''.10$; and, unless some of the contacts have been different in kind, such probable errors would be impossible with the optical means at the disposal of the French observers.

The value $8''.96$ is not much larger than that which was obtained from a comparison between the meridian observations of *Mars* made, in 1862, at the Northern Observatories and the Southern Observatories of the Cape and Melbourne; nor is it much larger than the value which will result from a discussion of similar observations made at the same Southern Observatories in 1876. The observations made in 1876 are now under discussion by the authorities at the Washington Observatory, and I had hoped that, long before this time, the result might have been in the hands of astronomers. I have, however, discussed the observations sufficiently to express an opinion that the observations require a value of the solar parallax of about $8''.95$ to satisfy the equations of condition.

I should be prepared to attach very great weight to the value $8''.96$ thus deduced from a discussion of these French durations, were it not clear to me that the equation (5) cannot legitimately be combined in one discussion with the equations (1), (2), (3), (4), (7), and (8), on account of the character of the contact observed at Ingress by M. Janssen.

The following is M. Janssen's account of the contact observed. (*Comptes Rendus de l'Académie des Sciences*, tome lxxx. [1875], p. 344):—

"Dans l'équatorial de huit pouces, dont la lunette est très-bonne, l'image de *Vénus* se montra très-ronde, bien terminée, et la marche relative du disque de la planète, par rapport au disque solaire, s'exécuta géométriquement sans aucune apparence de ligament ni de goutte. Mais il s'écoula un temps assez long entre le moment où le disque de *Vénus* paraissait tangent intérieurement au disque du Soleil, et celui de l'apparition du filet lumineux. Il y a là une anomalie apparente qui, pour moi, tient à la présence de l'atmosphère de la planète. J'ai fait prendre une photographie au moment où le contact paraissait géométrique, et sur cette épreuve le contact n'a pas encore lieu. M. d'Almeida a obtenu une plaque de quarante-sept photographies du bord solaire, qui conduit aux mêmes conclusions. . . .

"Enfin, vers l'instant du second contact intérieur, une éclaircie presque providentielle se produisit sur le Soleil, et nous pûmes, M. Tisserand et moi, prendre l'instant de ce contact, qui fut obtenu avec précision."

This description of the contact observed by M. Janssen at Ingress should be compared with that given by Admiral Mouchez, *Comptes Rendus*, tome lxxx. (1875), p. 616:—

"Mais, à mesure qu'approchait le deuxième contact, les deux parties extrêmes, plus visibles de l'auréole avoisinant le Soleil, tendaient à se réunir en enveloppant d'une plus vive lumière le segment encore extérieur de la planète, et cette réunion *anticipée* des cornes par un arc de cercle lumineux était rendue plus complète encore par un petit rebord très-brillant de lumière terminant l'auréole sur le disque de *Vénus*. Prévoyant dès lors qu'il y aurait une très-grande difficulté, sinon une impossibilité absolue, d'observer le contact géométrique, je changeai vivement le verre de couleur bleu pâle pour en prendre un plus foncé, à l'aide duquel j'espérais éteindre cette auréole et ces lueurs accidentelles, mais ce fut inutilement: l'auréole restant toujours visible, je fus obligé de reprendre le verre primitif.

"Dans de semblables conditions, *je dus prendre comme heure du contact, non pas la réunion des deux cornes ou contact géométrique, mais bien le moment où le disque du Soleil ne me parut plus déformé par la lumière brillante qui enveloppait la planète au point de contact.*

"J'ai noté une différence de temps très-sensible entre l'instant où j'ai cru que ce contact pouvait avoir lieu et celui où j'ai acquis la certitude qu'il avait eu lieu.

"Cette observation me paraît donc comporter beaucoup moins d'exactitude que celle des contacts intérieurs du Soleil et de la Lune dans les éclipses totales ou annulaires que j'ai eu l'occasion d'observer. Dans ces éclipses, en effet, il m'a toujours semblé possible de déterminer, avec la précision d'une fraction de seconde, le moment de la rupture ou de la formation de l'anneau. . . .

"Je dois m'empresser d'ajouter que mon collaborateur M. Turquet, avec un excellent équatorial de six pouces, n'a pas vu l'auréole, et qu'il croit avoir obtenu des contacts d'une grande précision."

At the Egress I saw this "aureole" very distinctly, but when attention was concentrated on the contact I lost sight of it, and it did not interfere with my observation of the contact.

It is clear that the time recorded by Janssen for the contact would have been later by "un temps assez long," had he waited until the disturbance of the illumination of the Sun's apparent limb near the point of contact had ceased, and sunlight had reappeared near the point of contact.

And his Ingress-contact cannot, therefore, be combined with such observations of contact as those made by Mouchez, without a correction for the difference of phase observed. The necessity for such a correction of Janssen's time of contact at Ingress was pointed out by me in the *Notices* for 1876, December, before the accurate times of contact had been published, and when, therefore, it was impossible for me to be influenced by any knowledge

of the value of the solar parallax which would result from a discussion of these observations. The correction clearly required by Janssen's Ingress-contact will make the equation (5) agree more closely with all the other equations except (6), which contains observations made by Tisserand at the same station.

I am unable to find any details of the nature of the contact observed by M. Tisserand. But the residual error of the equation (6) is rather larger than that of the equation (5). It is clearly necessary to reject or correct this equation (5), and I fear, therefore, that it will be necessary also to reject the equation (6). It will be seen that the mean residual error for the equations (1), (2), and (8), with a factor of parallax $-2{\cdot}9$ is $+0''{\cdot}42$, whilst the residual error for the equation (6) with a factor of parallax $-2{\cdot}7$ is $-1''{\cdot}11$. The residual error of the equation (6), therefore, differs from the mean error of the equations (1), (2), and (8) by no less a quantity than $1''{\cdot}5$; and from the close agreement of the factors of parallax it is quite impossible to adjust this difference by any change in the adopted value of the solar parallax. Such a difference as $1''{\cdot}5$ is equivalent to an error of about 45 seconds of time in the observed contacts. It is about equivalent to the whole angular separation between the two phases of the "first appearance of any distinct disturbance of the illumination of the Sun's limb near the point of contact" and the "apparent contact" in a field of considerable brightness; and it would be more than equivalent to such a separation in a feebly-illuminated field where the irradiation would be smaller, and the "apparent contact" at Egress would be earlier and at Ingress later than in a bright field. Speaking from experience, I feel certain that M. Tisserand can never have attempted to observe contacts of the same character as Mouchez. I hope that an examination of the details of his observations will show that he has not attempted to observe at Ingress the "last appearance of any well-marked disturbance of the illumination of the Sun's limb near the point of contact," or at Egress the "first appearance of any such well-marked disturbance," and that it may appear clearly from *a priori* considerations, that his duration cannot be combined with the observations of durations from "real contacts," without a correction for the difference of phase observed.

Rejecting, therefore, on the grounds indicated, (5) and (6), we find from the remaining equations

$$\pi = 8''{\cdot}88.$$

This appears to be the smallest value of the solar parallax which can be fairly deduced from these observations of duration.

The residual errors of the equations are

$$+0''{\cdot}15, \quad -0''{\cdot}22, \quad +0''{\cdot}25, \quad -0''{\cdot}02, \quad -0''{\cdot}50, \quad +0''{\cdot}56.$$

If M. Tisserand's contact either at Ingress or Egress should clearly appear to be attempts at "apparent contact," then the rejection of both the equations (5) and (6) is absolutely necessary, and the value 8″·88 would be entitled to very considerable weight. On the other hand, should it appear that M. Tisserand's observations are at Ingress and Egress observations of the "last appearance" and "first appearance" respectively of any sensible disturbance of the illumination of the Sun's limb near the points of contact, then the value of the solar parallax given by these durations will be larger than 8″·88; but the probable error must be considerable.

It appears from M. Tisserand's paper in the *Comptes Rendus*, No. 12, 1881, March 21, that the value of the solar parallax 8″·86 found by Le Verrier from a discussion of the masses of the planets requires, if anything, a slight increase rather than a decrease.

The way in which all the more important investigations on the solar parallax have, when cleared from obvious mistakes, given results which group themselves around some such value as 8″·88 is certainly remarkable. The extreme values appear to be those derived from the meridian observations of *Mars* which give some such value as 8″·95, and the values deduced from the recent determinations of the velocity of light, with Struve's coefficient of aberration, which appear to point to some such value as 8″·80. The difference between these values, although greater than could be wished, is not very large, and the mean agrees very closely with the mean of the values which have been obtained from the Transits of *Venus* and from the planetary and lunar theories.

The Partial Eclipse of the Sun, December 31, 1880.

By J. Rand Capron, Esq.

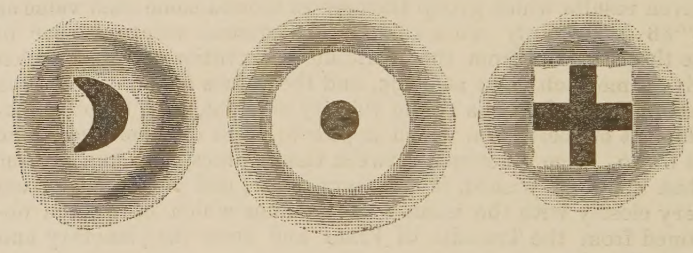
Upon examining the plates taken on this occasion, and described in the *Monthly Notices*, vol. xli. No. 3, p. 144, I have found three appearances in respect of which the photographic image differs from that seen by the eye, either unaided or with the telescope. As these are probably always, in a greater or less degree, present in photographs of similar objects, it may be useful to describe them.

1. Round the edge of the solar image, and in a stronger degree where it touches the Moon's limb, is the bright line of light seen in most photographs when black is contiguous to white. In portraiture it is well shown, edging a velvet dress, and is then frequently very sharp and definite. This effect—generally termed "photographic irradiation"—has been attributed by Lord Lindsay and Mr. Ranyard to an extension of chemical photographic action on the plate. Others, I believe, have attributed it to a thickening or building up of the chemicals

during development. Its appearance and causes have at all events been fully discussed elsewhere.

2. The second appearance not visible to the eye is a halo or ring of light seen some little distance from the Sun, and surrounding it except where the Moon's limb is in contact. Dr. Berwick, in *Nature* of October 30 and December 5, 1879, has described and figured this form of halo, as surrounding a full Moon which carried no trace of it to the eye. He was also led to infer that it was only found at full Moon, and not during the quadrants. Some experiments I made soon proved this to be a mistake. I found the same ring or halo, about a diameter distant from the object, to accompany the imperfect Moon, the Sun, and other bright bodies.

One photograph of a rising Sun shows it strong, though nothing of the sort was visible to the eye; and with the Moon images it seemed only a question of time of exposure. When by over exposure the Sun's image was reversed, the halo was so



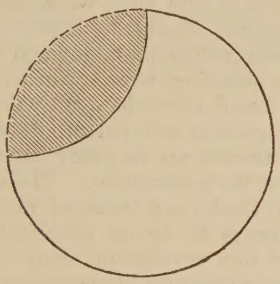
also. As a final experiment I photographed a card with apertures in it, covered by ground-glass; and obtained the following results, in which the shading indicates the halos which were in fact soft and diffused.

The circle gives, like the Sun and full Moon in the photographs, a truly circular halo. The cross gives a somewhat more complicated image; square internally, and tending to circular externally. The crescent gives a slightly oval halo, strengthening in intensity with the shape of the crescent. Between the horns, however, there is a marked absence of the halo. I am not aware how far this ring or halo surrounding, it would seem, all bright objects when photographed has been examined and explained. At present I am inclined to think it due to the lenses with which the pictures are taken, though very similar halos are frequently seen in certain conditions of the atmosphere or the eye. These last are, however, found in practice difficult to impress on the photographic plate with a short exposure, while with a long exposure the invisible halo always comes out.

3. The last point is a diffused halo or light spreading over the dark portion of the Moon projected on the Sun. This expands from the contact edge of the Sun and Moon, gradually

lessening in intensity to what would very nearly be the Sun's circumference, as indicated in the annexed diagram in which the dotted line shows its limit.

It is very clearly shown in one or two of the longer exposed plates, and at first strongly suggested the idea of a subjective phenomenon. I think, however, it is really due to that form of photographic defect distinguished from "irradiation" by the term "halation," and referred by Lord Lindsay and Mr. Ranyard to reflection from the back of the plate. It is peculiar that it should be convex and not concave in form, and that it should



not more plainly accompany the outer edge of the Sun. It must, however, be remembered that these pictures are, except as respects the Sun's visible disk, necessarily under-exposed; and that the Moon's limb on the Sun presents a much darker background on which such halation would be detected than the adjoining sky.

I may, I think, venture to add that all the foregoing phenomena are yet not entirely and perfectly understood, and fairly merit further attention as having an important bearing on celestial photography.

Guildown, Guildford;
1881, *March* 3.

Observations of the Red Spot on Jupiter. By J. Tebbutt, Esq.

I have observed several transits of the well-known Red Spot in *Jupiter's* southern hemisphere across the central meridian of the planet with the $4\frac{1}{2}$-inch Equatoreal. Two methods of observation have been employed: one with the aid of a position-micrometer, and the other by simple estimation of the time when the preceding end, the middle, or the following end, of the spot was equidistant from the visible eastern and western limbs of the planet. The micrometer method was as follows. The reading of the moveable screw for coincidence of the declination wires being obtained by contacts on each side of the fixed wire, the micrometer was revolved so as to bring the close transit-threads

parallel to the planet's belts. The declination wires were then separated till one of them coincided with the planet's eastern limb and the other with the western. In this way a determination of the equatorial diameter of the planet was obtained uncorrected for defective illumination. The wires being then set to half the measured diameter, the western wire was brought on the corresponding limb of the planet, and the eastern wire assumed to bisect perpendicularly the visible equatorial diameter. The wires being retained in this position, the transits were observed over the latter wire. Immediately on noting the coincidence of each part of the spot with the bisecting wire the position of the wires was changed by bringing the eastern wire on the planet's corresponding limb, and the western wire became thus in its turn the bisecting wire. The observed parts of the spots being again found coincident with the bisecting wire, the observations were regarded as verified. It will be seen that my estimated times of transit are in every instance later than those noted with the aid of the micrometer. The micrometer, which is the work of Messrs. Cooke and Sons, of York, the makers of the telescope itself, appears to be an excellent one as regards the screws, the value of one revolution being $29''{\cdot}717$. Each wire, however, I find from numerous observations to be about $1''{\cdot}2$ in thickness.

I have given the observed transits to the nearest tenth of a minute of local mean time, as it is impossible to observe with anything like precision. The impossibility of doing so is obvious from the recorded results of other observers.

Date of Transit.	Magnifying Power.	Transits, per Micrometer. Preceding End.	Middle.	Following End.	Transits, per Estimation. Preceding End.	Middle.	Following End.
1880.		h m	h m	h m	h m	h m	h m
Oct. 31	180				8 35·6	8 58·3	9 28·6
Nov. 2	180				10 6·4	10 34·4	11 2·4
7	180				9 19·8	9 41·9	10 12·0
10	187	6 30·4	6 58·6	7 35·8	6 41·6	7 5·6	7 41·3
15	55				6 0·7	6 25·0	6 49·2
17	187	7 21·8	7 48·3	8 14·8	7 30·0	7 56·3	8 25·3
24	187	8 8·6	8 35·7	9 3·4	8 16·9	8 44·4	9 8·7
Dec. 1	187	8 54·3	9 19·7	9 49·3	9 0·0	9 24·8	9 53·3
4	180				6 37·9		7 26·3
10		11 12·6	11 45·1	12 17·1	11 20·9	11 49·5	12 19·6
18	187	7 55·9	8 23·7	8 52·2	8 0·4	8 28·9	8 54·7
30					8 0·2	8 22·5	8 47·7

Observatory, Windsor, N.S. Wales:
1881, *January* 28.